LETTERS TO THE EDITOR

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[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

On the Connection between Chemical Constitution and Physiological Action

IN his letter to NATURE last week (p. 594), Dr. Blake considers that I have not only misunderstood the scope of his experiments, but have been led into error on account of my having no definite idea of the meaning of the term chemical constitution, which he thinks I have evidently confounded with that of chemical composition.

In regard to the first of these points, I shall be very sorry if, by mishap, I have not rightly understood, or have failed to appreciate at their true value, Dr. Blake's experiments (most of which were published before I was born), for I regard him as a true pioneer in the field of pharmacology.

The scope of Dr. Blake's researches, as defined by himself in the Report of the British Association for the Advancement of Science for 1846, was "fully to establish the law of the analogous action of isomorphous substances."

I should no doubt have described Dr. Blake's researches more correctly had I used the word "isomorphous" instead of translating it into popular language, for my translation undoubtedly does not give the full meaning of the word; but my whole address was an attempt to make a difficult subject as popular as I could, and I thought that I had sufficiently acknowledged Dr. Blake's priority by observing that the present epoch of pharmacology might be dated from his researches, although it was those of Crum Brown and Fraser which fairly started pharmacological investigation in a new direction. Perhaps Dr. Blake will be inclined to regard my shortcomings in regard to him more leniently if he will read over my address again, for, if he does so, I think he will see that if on my part I have failed to give him due credit, he on his part has completely misunderstood the whole drift of my address, which was to show the importance of chemical constitution as distinguished from chemical composition.

T. LAUDER BRUNTON

The Origin of Species

IT has already been pointed out by Mr. Evershed that the "physiological selection" of Dr. Romanes is identical with the theory outlined by me nearly two years ago in these pages (vol. xxxi. p. 4). As all the objections which have been raised apply equally to my theory, I may perhaps be allowed to give my answer to some of them; it will probably differ in some points from that promised by Dr. Romanes in the *Fortnightly*.

I quite agree with Mr. Wallace (in the *Fortnightly*) that it is only among the group of animals which have at least one common parent that the corresponding variations of the sexual organs which are required for physiological insularity would be likely to occur. But when he maintains that not more than two or three of such a group would reach maturity, and that therefore they would soon die out, he seems to me to forget that it is only on the average that the number would be so small. Many groups would be small, and would die out; exceptional families would be more numerous and more lucky; just as we can all point to human families where twelve or more children have reached maturity, though the average number of those who do so is under three in a family.

The survivors, more or less numerous, would generally not be scattered far from their common birthplace, so that their chance of finding one another would not be very small, especially if the sexual instinct was correspondingly modified, and this might well be the case from what we know of the connection between the psychical and physiological parts of the reproductive function. This presupposes some difference of smell, form, colour, &c., to enable an animal to distinguish those of its own family from the rest of the species, but this probably exists between any two animals.

They might thus be under no great disadvantage compared with the parent species, and they would have a counterbalancing advantage in the much greater adaptability to circumstances which a small group possesses. Any useful variation occurring in a large group, if not swamped by the effect of interbreeding with a large number of unimproved forms, must take many generations to modify the whole mass; while a similar useful variation occurring in one member of a small and physiologically isolated group could modify the whole group in a few generations. The existence of a six-fingered man in England would not appreciably modify the inhabitants in a thousand years, even if it was a slight advantage to have six fingers; while if a six-fingered man was introduced into an island with five other inhabitants, a fair proportion of the population would probably be six-fingered in three generations.

It is perhaps worth pointing out that the curious connection between colour and fertility, in which Mr. Wallace seeks for the explanation of the sterility of species, follows at once as a corollary from the doctrine of physiological selection. For, apart from any special modification of the sexual instinct, all animals seem to prefer to breed with those of their own colour, and hence *any* change of colour in the isolated family would be an advantage, and would indeed remove the one disadvantage under which such a family lies. So a change of colour, otherwise useless, would in such cases be preserved, and be found accompanying sterility with the parent species.

Another of Mr. Wallace's objections seems to me a strong argument in favour of my (and Dr. Romanes's) theory. He says that some animals, not only of different species but of different genera, can produce hybrids, and he instances the pheasant and black grouse. Now this is just what we ought to find on our theory, and ought not to find on any other. If either structural divergence or divergence in colour produces infertility, then the pheasant and the black grouse should be sterile, since they differ more, both in structure and colour, than many sterile pairs. But if species are produced sometimes by physiological isolation, but sometimes by other causes, such as geographical isolation, spontaneous distaste (not disability) for pairing, or even unaided natural selection, then those species which have been produced by aid of any of these latter processes will be fertile in spite of any ordinary amount of divergence, since nothing has occurred to render them otherwise; while those which have been formed by physiological isolation will be sterile even though they have hardly diverged at all. We cannot tell, without assuming what I am trying to prove, what form of isolation has been at work, except in the case of island species; but we can tell that there ought to be both very divergent fertile forms and slightly divergent non-fertile forms, and this is the case.

It has also been objected that the gradual increase of sterility, as we pass from different species to different genera and families, proves that divergence produces sterility. But it would exist on my theory; for if physiological isolation, more or less complete, occurs before each species is formed, it will have occurred at least twice between the members of two genera, and more often between those of two families. If B is separated from A by being nearly infertile, and C from B in the same way, C is likely to be still more infertile with A. But in some cases geographical or other isolation takes the place of physiological isolation, and then any number of successive divergences may occur without any accompanying infertility.

It has been said (I have lost the reference) that a certain amount of sterility has resulted in some cases from the divergence produced by artificial selection. It may be so. But on my theory, physiological isolation, the spontaneous occurrence of a fertility circumscribed by the boundary of common parentage, must be of very common occurrence, since it must have occurred not only once for each of most of the recognised species, but many more times when the resulting species has died out, and in some cases where the two species, though still existing, have not diverged in any way so as to suggest to observers that they are not one, (just as many island species do not differ perceptibly from those on the mainland). If spontaneous physiological isolation is so common, it would be certain to occur, at any rate in its commoner partial form, among the great variety of our domesticated animals, even if, as I believe, ordinary variation has no tendency to produce it.

EDMUND CATCHPOOL

Friends' Institute, E.C., October 13

Note upon the Habits of Testacella

BETWEEN four and five months ago I found eleven specimens of this slug upon a low wall surrounding the garden of a house

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near the Oxford University Parks, and on the following day I captured eleven more in the same place. There had been exceptionally heavy rain, extending over some days, immediately previous to those on which I found the specimens, and it therefore seems probable that these animals are driven out of the earth when it becomes sodden with moisture. Thus it is possible to account for the capture of a very unusual number of specimens, for, as far as I can learn, the species has hitherto only been met with singly in this locality.

I have also ascertained what happens to the animals when the earth in which they are contained becomes hard and dry from the loss of water. A few of the twenty-two specimens were killed and hardened, and the remainder were put in a box containing earth, in which they buried themselves. In the press of other work the box was neglected, and remained untouched in my laboratory until to-day, the earth having quickly dried into a hard cake. To-day I emptied the box, and fully expected to find the slugs dried up dead, but to my surprise I found twelve specimens alive, each encysted in a thin transparent capsule formed of the hardened mucous secretion of the animal's skin. The body was contracted, and oval in shape, but it had been so completely protected from evaporation that there was no noticeable reduction in bulk after these hottest months of the year, during which water had been entirely withheld. One or two specimens had died almost immediately after capture, and a few escaped, so that all those which had been exposed to the heat and dryness in the box had become encysted, and survived in apparent health. EDWARD B. POULTON

Wykeham House, Oxford, October 19

Lepidoptera and Migration

THE subject of migration in connection with Lepidoptera is beginning to receive some attention at the hands of our best lepidopterologists. I am decidedly of opinion that the abundance or scarcity of many species of Lepidoptera is largely regulated by migrations from abroad. Last year our southern shores were visited by an abnormal number of rare *Sphingidæ*, but this year there have scarcely been any records of captures published concerning them. It would be interesting to know what are all the influences which cause these migrations, and if there is a periodicity to the phenomenal occurrences.

Birmingham, October 12 W. HARCOURT BATH

The Earthquake of October 16 in the Vosges, &c.

MAY I be allowed to call attention to the fact that Alsace lies on the direction of the great circle, "boundary of Tertiary formation of the United States," mentioned in my letter which appeared in your number of the 14th inst. (p. 570), and furthermore that Strasburg has been repeatedly shaken since 1355, the first date which I found recorded as having been marked by a shock. It is quite true that the interval between that shock and the next recorded (1556) was 201 years; but the greatest subsequent interval, that between the shocks of 1577 and 1655, was only of 78 years. This interval represents a multiple of 13, being = 13 × 6. The interval of 13 is of frequent recurrence, as I purpose to show in a paper which I have about terminated on this question of intervals and periods of earthquakes.

J. P. O'REILLY

Royal College of Science for Ireland, Stephen's Green,
Dublin, October 23

RECENT ORNITHOLOGICAL WORKS

THE future student of British birds ought to have little difficulty in working out the distribution of species within the shores of Great Britain, so much excellent work having been done in the way of local lists during the last few years, and certainly one of the most useful will be the little work on "The Birds of Cumberland and Westmoreland," just issued by the Rev. H. A. Macpherson and Mr. W. Duckworth.[1] The situation of these two counties is interesting, especially to the student of migration, and the notes on the passage of water-birds and sea-birds are particularly good. The completeness of the information, and the concise and simple form in which it is conveyed, render this small book a model of what a faunistic work should be, and it forms a worthy accompaniment to the many excellent county lists of birds which have appeared in England during the last twenty years. It would be well if every expiring species in Great Britain had had its death-song as well sung as is the case with the Dotterell, by Mr. F. Nicholson, in the present work. While Protection Acts are spreading their ægis over many birds in the breeding season, so that the numbers are visibly increasing, and the enlightened care of a few landed proprietors aids the work of bird-preservation, there are still a certain number of species whose nesting days in this country are numbered, and which, like the Great Bustard and the Bittern, are doomed by the inexorable advance of civilisation to seek less over-crowded countries in which to breed. The (too probably final) breeding of the Dotterell in Cumberland is therefore appropriately described by Mr. Nicholson, who has himself taken the eggs in the county. An excellent account is likewise given of the breeding of the Pied Flycatchers.

We learn with some surprise that the White-headed Long-tailed Titmouse of Scandinavia, the true *Acredula caudata* (Linn.), "may be detected in Cumberland in mid-winter," when "the appearance of a flock of adults in their snow-white caps is refreshing to an insular observer." We should like to see some of these Titmice, and may state that an example is a desideratum to the national collection, where we should be glad to receive a specimen. Our experience in France, where we have shot *all three* races of the *Acredula caudata*, is that it is impossible to detect the difference of the forms when in the open, though a difference in note led to our recognising *A. irbii*. We can only consider the true *A. caudata* to be a very occasional, though not impossible, migrant to our shores, and we by no means sympathise with the authors of the "Birds of Cumberland" in their suppression of the name of *Acredula rosca* for the British Long-tailed Titmouse, from a dislike to the "needless multiplication of species." As regards Great Britain the facts are perfectly plain. The resident *Acredula* is always recognisable, and the white-headed form is only a very occasional winter visitant, and however much they may interbreed in the Rhine Provinces or elsewhere on the Continent, there is nothing of the kind in England, where perfect differentiation exists; and therefore to say that our English Long-tailed Titmouse should be called *Acredula caudata* of Linnæus, is a mistake and nothing else, for that name belongs to the Swedish form. Those ornithologists who continue to do this suppress a most interesting fact in nature, viz. that the isolation of the British Islands from the rest of Europe has produced a well-marked modification in the colour of some of our birds, amounting in certain instances to a subspecific value. The same reasoning applies to the Coal Titmouse, where our authors state that "British specimens have *generally* olive backs, as contrasted with the slate-gray backs of typical German specimens, but intermediate forms occur." This is not our experience. In summer plumage, when the olive-brown tips and the feathers become shed, the back of the English Coal Titmouse is gray, and then it is difficult to tell it from a summer-plumaged *P. ater* from the Continent. But if two winter-killed birds are compared, the difference between the British and Continental specimens is very strongly marked, for the back in the latter remains gray, whereas in the British form it is olive-brown. Having been the first, as we believe, to detect this modification in the British form, we have, ever since we first gave it the name of *Parus britannicus*, assiduously collected a series of specimens in the British Museum, and we have never seen reason to modify our original opinion, nor have we yet seen the intermediate forms for which our authors vouch.

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[1] "The Birds of Cumberland critically studied, including some Notes on the Birds of Westmoreland." By the Rev. H. A. Macpherson, M.A., and William Duckworth. (Carlisle, 1886.)

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